

## TOWARD A COMPARATIVE PSYCHOLOGY OF LEARNING<sup>1</sup>

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**I**N the early years of this century, when the experimental study of animal intelligence was just getting under way, many different species were brought into the laboratory. From the very beginning, to be sure, there was some preference for the higher animals, but, on the whole, early interest ranged rather widely up and down the phylogenetic scale. Before long, however, the scope of research narrowed. Attention became fixed on a small number of mammalian forms, which were chosen primarily for reasons of convenience, and treated as representative of animals in general, with the cheap and docile rat easily leading all the rest.

A set of curves which nicely illustrates this trend was published some years ago by Beach (1950). Based on a count of papers appearing between 1911 and 1948 in the *Journal of Comparative and Physiological Psychology* and its forerunners, the *Journal of Comparative Psychology* and the *Journal of Animal Behavior*, the curves show how interest in the rat mounted rapidly, while interest in the submammalian forms declined. In the thirties, a stable pattern emerged, about 60% of the papers dealing with the rat, about 30% with mammals other than the rat, and about 10% with the lower forms (the submammalian vertebrates and the invertebrates). If we make the computations required to bring these curves up to date, we find no significant change in the state of affairs decried by Beach a decade ago. Please note that Beach's curves are based on all papers published in a single journal. If we count only the papers on learning, and look at a broader sample of journals, the effect becomes even more striking. About 90% of our work on animal learning has been done with the rat.

Of course, specialization has its advantages, and, if the process of learning were essentially the same in all animals, it would be rather improvident of us

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to diffuse our efforts over the phylogenetic scale; but the only way to find out whether the process of learning is essentially the same in all animals is to make some careful studies at widely separated points in the scale. We have not made such studies. It is our willingness to *assume* that the process of learning is essentially the same in all animals which has been responsible in large measure for our concentration on the rat.

The assumption comes to us from Darwin, who was interested in psychological as well as in structural continuity. For Darwin, novelty was incompatible with continuity, and his purpose was to demonstrate that any seemingly unique property which made its appearance in the animal series really was not unique at all—that some hint or promise of it always could be discovered at an earlier point in the series. On the basis of the only evidence available to him—the tall tales of naturalists and zookeepers—Darwin decided that phylogenetic differences in intelligence were differences “of degree and not of kind.” Not even in the intelligence of man could Darwin find anything new. Capacities which *seemed* peculiar to man, he explained, could be traced to man's use of language, which itself—“half-art and half-instinct”—clearly bore “the stamp of its gradual evolution” (1871, pp. 105–106). Darwin's formula for bridging the gap between man and the infrahuman animals has a contemporary flavor; it finds wide use even today.

The early comparative psychologists were not entirely convinced by Darwin's arguments. Critical of his anecdotal evidence, and proud of their own new methods, they were determined to have a look for themselves; but their skepticism did not last long. Observing a variety of animals in problem boxes, mazes, and discriminative situations of one sort or another, they were more impressed by the similarities in behavior than by the differences, and quickly succumbed to the Darwinian view. Thorndike, himself, showed the way. At the outset, his contempt for certain features of Darwin's position was undisguised—“man was no more an animal with language,” he wrote, “than an elephant was a

cow with a proboscis" (1898, p. 87)—but, prepared though he was to find important phylogenetic differences in mode of learning, he found none. His very earliest observations led him to the "working hypothesis" that the process of learning was essentially the same throughout the phylogenetic series. All animals, for Thorndike, were "systems of connections subject to change by the law of exercise and effect" (1911, p. 280), differing only in the particular connections formed and in the ease of connection. The influential Watson took the same position, although he rejected the law of effect and tried to get along with exercise alone—"in passing from the unicellular organisms to man," said Watson, "no new principle is needed" (1914, p. 318)—and gradually Thorndike's working hypothesis became transformed into an article of faith. The transformation is reflected in Beach's curves. As a working hypothesis, the proposition that learning is essentially the same in all animals led to the study of many animals. As an article of faith, it led, by the principle of least effort, to concentration on one.

Specific illustrations of the contemporary faith are not hard to find. Dollard and Miller base their account of human personality and psychotherapy on the assumption that, as they put it, "any general phenomena of learning found in rats will also be found in people" (1950, p. 63). They admit that "people may display additional phenomena not found in rats," but at no point in their book can one find any indication that they take the possibility very seriously. What they refer to as "the higher mental processes" of man they explain in terms of mediating responses, primarily linguistic in character, to which the general laws of stimulus-response connection are assumed to apply.

Another interesting example of the contemporary faith was provided by Skinner a few years ago, when, in an address to the Eastern Psychological Association, he attempted to justify his disregard of the standard statistical procedures. Those procedures are necessitated, he said, only by the fact of individual differences, and they lose all point when individual differences are eliminated "in advance of measurement." To demonstrate the extent to which his own method eliminates irrelevant sources of variability, he presented, among others, the curves shown in Figure 1. The curves were made by three animals trained in three different laboratories on a multiple fixed-interval-fixed-ratio schedule of reinforcement—one by a pigeon, one by a rat, and one

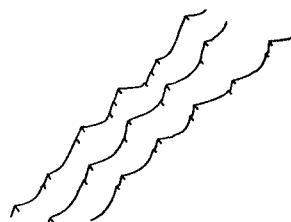


FIG. 1. Cumulative-frequency curves for pigeon, rat, and monkey (not necessarily in that order) trained on a multiple fixed-interval-fixed-ratio schedule of reinforcement. (From Skinner, 1956.)

by a monkey. "Pigeon, rat, monkey, which is which?" Skinner asked, and he answered:

It doesn't matter. Of course, these three species have behavioral repertoires which are as different as their anatomies. But once you have allowed for differences in the ways in which they make contact with the environment, and in the ways in which they act upon the environment, what remains of their behavior shows astonishingly similar properties. Mice, cats, dogs, and human children could have added other curves to this figure. And when organisms which differ as widely as this nevertheless show similar properties of behavior, differences between the same species may be viewed more hopefully. Difficult problems of idiosyncrasy or individuality will always arise as products of biological and cultural processes, but is it the very business of the experimental analysis of behavior to devise techniques which reduce their effects except when they are explicitly under investigation (1956, pp. 230-231).

The function of these curves, the context suggests, was not to demonstrate that the process of learning is essentially the same in pigeon, rat, and monkey; that was assumed. The function of the curves was to demonstrate the power of the method, which was found in the fact that it *reveals* the essential similarity of the three animals despite substantial sensory and motor "idiosyncrasies."

Whatever Skinner's purpose, his curves do, of course, show some interesting similarities in the learned behavior of quite different animals, and evidence of the same sort has been accumulating since the turn of the century. A comparable set of curves dating back to the years between 1901 and 1904 is shown in Figure 2. Like those of Skinner, they represent the work of three independent investigators working with three different animals—a monkey, a rat, and a bird (in this case, a sparrow). The apparatus employed was the Hampton Court maze, as popular in its time as Skinner's box is today. "Monkey, rat, sparrow, which is which?" it was asked at the turn of the century, although the question went to the nature of phylo-

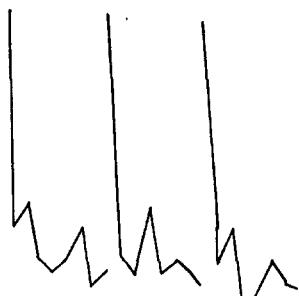


FIG. 2. Relative-error curves for sparrow, rat, and monkey (not necessarily in that order) trained in a Hampton Court maze. (After Small, 1901; Kinnaman, 1902; and Porter, 1904.)

genetic differences rather than to the power of the maze method, and the answer came: "It doesn't matter." That answer is no more warranted now than it was 50 or 60 years ago. Attractive as the Darwinian hypothesis may seem, it should not be accepted on the basis of superficial resemblances of this sort. The resemblances may, in fact, be more than superficial, but we shall never know until our level of inquiry becomes more than superficial.

Our skepticism must, of course, be based to a considerable extent on some guesswork about the significance of phylogenetic differences in brain development. Thorndike, too, was interested in brain development, but he was inclined to attribute to it a purely quantitative significance; he early speculated that brain development meant merely the capacity for a greater number of connections and greater efficiency of connection. Even in recent years this kind of thinking has been common—for example, Miller (1951) has suggested that brain development may have no more significance than the addition of banks of keys to an automatic calculator—but brain development is more than an increase in the mass of brain tissue. New structures, such as the cortex, have appeared, which suggest the possibility of new functions. It is interesting to note, by the way, that the acceptance of the Darwinian hypothesis was accompanied by a growing disregard for the facts of brain structure. After giving up their inquiry into animal consciousness, the early comparative psychologists were at some pains to establish that there still remained to them an area of investigation different from that of the zoologists, and that their methods and findings were as important as those of the zoologists. If some primitive experiments with a maze or a problem box suggested that the process of learning

was the same in monkey and sparrow, why then perhaps the difference in brain development had no fundamental significance. That was the attitude which Watson expressed when, speaking of man's frontal lobes, he said that "simply because we have the tissue" it does not necessarily "have a life and death significance" (1914, p. 320). To be sure, we must agree with Watson in principle. If extensive and detailed investigation at a number of widely separated points in the phylogenetic scale reveals no fundamental difference in the mode of learning, we must sooner or later conclude that differences in brain structure have no relevance for mode of learning; but we should not be quite as easily persuaded as Watson.

Of course, not everyone *has* been as easily persuaded. From time to time, skeptical voices have decried the facile acceptance of Darwin's hypothesis and have urged the development of a systematic comparative psychology, but they have not often been able to propose a reasonable way of going about it. One of the most respected advocates of comparative research was Yerkes (1917), whose approach was that of the mental tester. Yerkes' idea was to develop a standardized situation appropriate to the sensory-motor capacities of a variety of species, and to order the species on the basis of their performance in it. This seems to be the common conception of comparative psychology even today. For example, following the lead of Harlow (1949), a number of investigators recently have been comparing raccoons, children, chimpanzees, and monkeys of varying description in terms of their rates of improvement in long series of discriminative problems. No wonder there has been little enthusiasm for comparative psychology so conceived. When I mention to someone that I am making comparative studies of learning, his first question is apt to be, not "How are you going about it?" but "How do you know that your measures are comparable?" He assumes automatically that I am comparing the animals in terms of certain absolute measures of performance from which I propose to infer differences in ability, and he wonders, then, how I can ever be sure that differences in performance *are* due to differences in ability rather than to sensory, or to motor, or to motivational differences. I sometimes find difficulty in convincing such a person that there is another kind of comparative research.

Suppose that I introduce a target into the living tank of a fish and reward the animal with a pellet of food for pushing against it.<sup>2</sup> The measure of performance is latency of response—the number of seconds which elapse between the introduction of the target and the animal's response to it—and I plot a curve showing change in latency over a series of reinforced trials. After the latency of response has fallen to a stable level, I terminate reinforcement and plot the progressive increase in latency which ensues. This procedure is directly analogous to one which has been used rather widely with the rat, and the results obtained—the acquisition and extinction curves—are quite similar to those for the rat. The absolute latencies may be very different in the two animals, but the *relation* between latency and trials is very much the same under conditions of consistent reinforcement and nonreinforcement, and it is the relation in which I am interested. In the past 50 or 60 years, we have begun to work out many such relations for the rat, and our theory of learning in the rat represents an effort to find order and meaning in those relations. I am interested now in the extent to which similar relations are to be found for the fish, because I am interested ultimately in determining whether a single theory will fit both animals.

Here, then, is the plan: Taking the much-studied rat as a point of departure, I select for comparison another animal—a fish—which is different enough from the rat to provide a marked phylogenetic contrast, yet similar enough to be studied under analogous conditions. The two animals are not to be compared in terms of their absolute scores in some standard apparatus. Work with the fish, like work with the rat, is to be directed at the discovery of functional relations. Its goal is a theory of the fish with which to compare the theory of the rat (Bitterman, Wodinsky, & Candland, 1958).

This conception of the comparative psychology of learning (as an attempt to determine whether learning in different animals may be understood in terms of the same set of laws or whether different laws are required) is not a new one. It was implied in the writings of Thorndike, and in the work of a small number of subsequent investigators, such as Schneirla (1946), who continued to take the comparative problem seriously. It was stated quite clearly by Hull (1945), although he himself did

not give much weight to the possibility that different laws might operate at the different phylogenetic levels. As yet, however, there has been no explicit attempt to deal with a methodological difficulty which has seemed to stand between this conception and fruitful research—a difficulty similar to that by which the mental-test strategy is utterly defeated.

To the extent that functional relations of the same kind appear in fish and rat no one is troubled. The principle of parsimony leads us all to assume—though perhaps incorrectly—that the same process of learning is operating in the two forms. Suppose, however, that different functional relations appear. We may not be as ready then to infer that the underlying processes of learning are different. We may wonder, and with good reason, whether some peripheral factor—sensory, or motor, or motivational—is responsible for the discrepancy, and we may despair of ever being able to control such variables in comparative experiments. How should we hope to find a situation whose sensory and motor demands upon our fish are equivalent to those of the runway or the bar pressing apparatus upon the rat? How should we hope to produce in our fish levels of motivation comparable to those which commonly prevail in experiments with the rat? Fortunately, the problem is not an insoluble one. While the prospects for *control by equation* are slim indeed, there is available a perfectly suitable alternative, which we may speak of as *control by systematic variation*.

Consider, for example, the paradoxical relation between consistency of reinforcement and resistance to extinction which has been established in a variety of experiments with the rat, and suppose that a like effect fails to appear in an analogous experiment with fish—that a partially reinforced group of fish extinguishes *more* rapidly than a consistently reinforced group. Are we led at once to the conclusion that different processes of learning operate in fish and rat? Certainly not. We must consider carefully the possibility that the relation between consistency of reinforcement and resistance to extinction is influenced by certain contextual variables, such as drive level or effortfulness of response, and that in our experiment with the fish we have permitted one of these variables to take on a value well beyond the range prevailing in experiments with the rat. Interactive interpretations of this kind can be tested without great difficulty. It does

<sup>2</sup> For methodological details, see Longo and Bitterman (1959).

not matter that we are unable to equate such variables as drive level in fish and rat; we know how to manipulate them and that is enough. If, working with *either* animal, we find the same relation between consistency of reinforcement and resistance to extinction at diverse levels of drive, the possibility that a difference *between* animals may be explained in terms of drive level is ruled out, and other interactive hypotheses can be tested in like manner. It may be well to note that the example which I have chosen is more than hypothetical. The initial resistance to extinction of *Tilapia macrocephala* (the fish with which my colleagues and I have been working) is greater after consistent than after partial reinforcement, and the outcome is the same with spaced or with massed trials, after relatively small or relatively large amounts of training, and at different levels of drive (Longo & Bitterman, 1960; Wodinsky & Bitterman, 1959, 1960). My main concern here, however, is with general strategy.

Given the decision to study some simple animal (such as the fish) under conditions analogous to those which have been used for the study of learning in the rat, with functional relations to provide the basis for comparing the two forms, there remains the question of where to begin. Intuitively, there seem to be many points at which the learning of fish and rat might be compared with profit, and my only verbalizable guiding principle has been to begin with conditions whose effects at the level of the rat have resisted analysis in terms of our simplest constructs. Contemporary learning theory seems to me to underestimate the rat, but I have developed a certain sentimental attachment to it, and the idea has occurred to me that a more comfortable place might be found for it at some earlier place in the animal series. That is, the rat phenomena which embarrass the theory simply may not appear in some more primitive form. Consider again the paradoxical effect of partial reinforcement on resistance to extinction in the rat, which once it seemed possible to explain in terms of stimulus generalization. The neo-Guthrians have attempted to bolster the failing generalization principle with a rather dubious habituation principle, but even the two principles together cannot deal adequately with all of the data. Suppose, however, that partially reinforced fish showed the paradoxical effect when and only when they could be expected to do so on the basis of these simple principles. We might then

be willing to conclude that contemporary S-R theory is appropriate at the level of the fish, although new processes of learning came into operation at the level of the rat. As it happens, the paradoxical effect does not appear in our fish even where these simple principles suggest that it should; but I am only half serious, of course, about the possibility of finding an exemplary S-R creature in the lower reaches of the phylogenetic scale. I do, however, have considerable confidence in the notion that the mammalian phenomena which confound the theory are less likely to appear in more primitive species than are those which have suggested its basic postulates, and I have chosen to begin with the former because I am interested in maximizing the probability that functional differences will be discovered if they do in fact exist.

If the much-studied rat is to provide a phylogenetic frame of reference for comparative work with other animals, the choice of starting point also must depend on the structure of existing literature on the rat. Despite the volume of that literature, it is not exactly rich in well-defined functional relations, probably because so much of our past effort has been devoted to a search for crucial experiments designed to resolve certain very broad theoretical problems which arose out of the very earliest work on animal intelligence. I certainly do not regret this effort. The problems are real ones, and we have gained much insight into them. It is significant, I think, that nothing much in the way of well-defined functional relations has been forthcoming even from those who have rejected the traditional questions and advocated a vacant empiricism; in the hands of the Skinnerians, for example, batteries of expensive automatic equipment have yielded little more than an idiosyncratic assortment of kymograph tracings scarcely capable of quantitative analysis. Theoretical concern may not lead of inevitable bar. The position is easier to defend that a mature investigator does not take the trouble necessity to the plotting of functional relations, but certainly it must not be thought to constitute an to make systematic measurements unless they promise to clarify some larger problem. In any event, the theoretical concern which I am here attempting to delineate encourages the discovery of functional relations—both in new animals and in old. I must note that my interest in comparative work with new animals has not made it possible for me to give up work with the rat. My original notion—that I

would study only the new animals and compare the functional relations obtained with those already available in the literature for the rat—proved far too simple. Sometimes an experiment with the fish gives rise to questions about the rat for which there are no adequate answers in the literature, and I may be too interested in those answers to be content to wait until somebody else happens to supply them.

Exploratory work with new animals is not for the impatient; the ratio of achievement to effort, at least in the beginning, is rather small, which should not be surprising in the light of the history of research on the rat. One must learn how to keep each new animal in the laboratory, how to motivate it, and something about its perceptual and motor capacities, before the quest for an appropriate set of experimental conditions even can be begun. The difficulties are many, and failure much more common than success.

In our work with the fish, as it happens, my colleagues and I have progressed at a quite satisfactory rate. We have succeeded in developing a set of efficient, objective techniques, well suited to a variety of species, which permit us to attack the problems of learning in fish on a broad front. One of these techniques, which has been mentioned already, and to which we have given most of our attention thus far, involves the presentation of a target at which the animal is trained to strike for food reward. After some preliminary work with a crude, mechanical system (Haralson & Bitterman, 1950), we developed a more sensitive and reliable electronic one (Longo & Bitterman, 1959). The present target is a disk of metal mounted on a light rod which is inserted into the needle holder of a crystal phonograph cartridge, and the amplified output of the cartridge is used to operate a set of relays which record and reward response. A single-target apparatus may be used either in Thorndikian or in Skinnerian fashion. That is, one may measure the latency of response in discrete trials, each trial beginning with the introduction of the target and terminating with its removal; or one may measure rate of response to a continuously available target. With two targets introduced simultaneously, choice may be measured, as in the T maze or jumping apparatus. More recently, we have developed two additional techniques, one for the study of escape and avoidance, which is so closely patterned after Warner's shuttle box as to require no further de-

scription here, and one for the study of classical conditioning (Horner, Longo, & Bitterman, 1960a, 1960b). In our classical conditioning situation, the US is brief shock, and a paddle inserted into the water detects the generalized response which the CS soon begins to elicit. Again a phonograph cartridge plays an important role, its amplified and integrated output driving a counter which provides an objective measure of response-magnitude. The scope of comparative research made possible by these three techniques is considerable, but they came, it must be emphasized, only after several years of trial which yielded very little in the way of useful data. Our work with other animals often has progressed at a much less satisfactory rate, and in some cases we have made almost no progress at all.

An invertebrate to which we have devoted a good deal of fruitless effort is the Bermuda land crab, *Gecarcinus lateralis*, which is quite easy to keep in the laboratory. It lives at 78° F. on some moist sand in a small fish tank, and it does nicely on half a peanut, some lettuce, a bit of raw carrot, and piece of eggshell once each week. It even does nicely for months on no food at all, which was a source of some disappointment to us, since we had been led to believe that its appetite was good. Failing at first to elicit consistent interest in food under our experimental conditions, we turned to escape. We tried light and heat which did not prove suitable, and then shock, which proved quite disorienting, often causing the animal to drop most of its limbs. At last, after some months of fruitless effort, we hit upon immersion in distilled water, which (probably because of its interference with salt regulation) seemed to produce a rather sustained effort to escape.

The uppermost drawing in Figure 3 illustrates the earliest form of the apparatus which we then proceeded to develop, an adaptation of a long, narrow fish tank that we happened to have on hand. We painted one end black, the other end white, and made a clear plastic starting compartment for the center, with two yoked guillotine doors that were raised simultaneously to permit choice. A coarse wire mesh ramp at one end led up out of the water to the home cage of the animal, also equipped with a guillotine door. The other end offered no escape from the water, but a dummy cage was set there for visual balance. In this situation, our crabs rapidly developed a preference for the positive side, where-

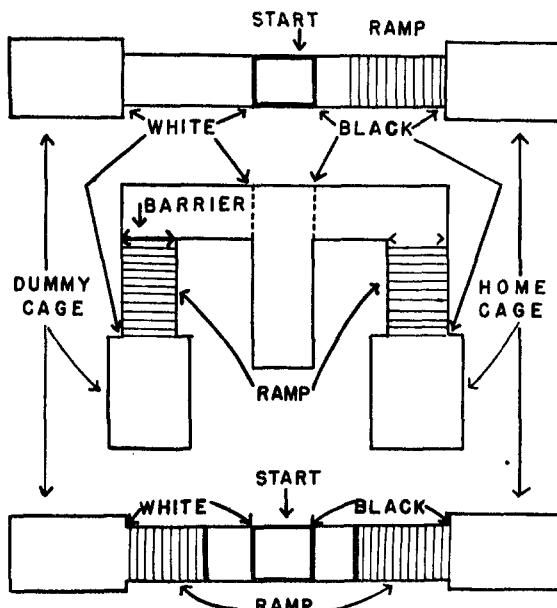


FIG. 3. Apparatus for the study of learning in the crab.

upon we constructed the more elaborate apparatus sketched in the central portion of Figure 3. There was a stem leading to a T-shaped choice-point, and there were ramps on both sides, each of which could be blocked off by a barrier which was not visible from the choice-point. In this apparatus, the crabs did not learn at all; they would remain for long periods in the stem, scrabbling ineffectively at the front wall. When we removed the stem and installed a starting compartment of the earlier kind, performance improved, but the elbows still seemed to make for considerable confusion. At last, we removed the end-sections of the apparatus, returning to the original linear pattern, and the animals regained their earlier efficiency. Several months were lost in making the circle. The new apparatus, sketched in the lower portion of Figure 3, does, of course, have features that the earliest did not. There are two ramps, with a guillotine door before each. Both doors are down to begin with, and the door on the correct side is raised only after the subject has made a correct choice.

Under these conditions, it was possible for us to complete our first formal study of learning in the crab—an experiment on habit reversal (Datta, Milstein, & Bitterman, 1960). In the course of that work, however, we had occasion to become dissatisfied with our technique. Clearly, we were able to achieve considerable control over the behavior of our animals, but the relatively low accuracy of our

control group (which was not reversed) suggested the possibility that a higher level of drive should be sought. Now we are using dilute solutions of acetic acid in place of the distilled water. The acid strikingly improves both speed and accuracy of performance, but the animals do not survive many experimental sessions. Perhaps we shall be able to find a concentration strong enough to motivate the animals satisfactorily yet weak enough that it will not impair their health, or perhaps we shall be able to find a buffering procedure to promote post-experimental recovery from the effects of immersion in strong solutions. So the search for a suitable motivating technique continues.

Another arthropod with which we have been working recently is the blowfly, *Phormia regina*. Our interest in this animal was stimulated by the physiological investigations of V. G. Dethier (for example, Dethier & Bodenstein, 1958), who taught us how to breed it and how to keep it in the laboratory. We have studied the fly in a simple runway and in a number of choice-situations, such as that shown in Figure 4. The work is done with harnessed individuals. The subject is anesthetized with  $\text{CO}_2$ , and a leash of light thread with a loop in its distal end is fixed to the dorsal surface with a bit of wax. The operation does not impair flying ability, but flight is limited in the experimental situation by a length of wire threaded through the loop at the end of the leash. In the apparatus sketched in Figure 4, the wire is curved, permitting the animal to alight only on one or the other of the two targets, one of which is baited with sugar solution. After innumerable variations of method and procedure, our efforts to develop situations suitable for systematic studies of learning in the blowfly seem finally to be meeting with a certain degree of success.

An animal with which we have had no success whatsoever as yet is the earthworm. For a long

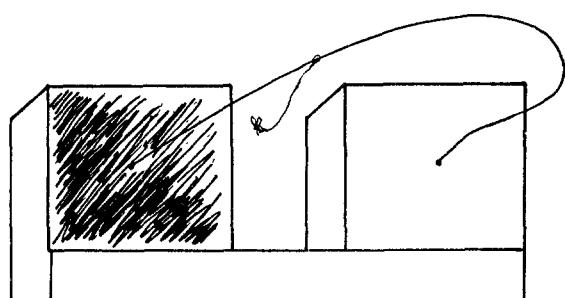


FIG. 4. Apparatus for the study of learning in the fly.

time I have been wanting to make a systematic study of the process of classical conditioning in a really primitive animal, and for the kind of work I have in mind it is not enough merely to look at the animal and try to decide whether or not it twitches when the CS is presented; an objective measure of response-magnitude is required. A few years ago, I came upon a paper by Galambos (1939) which seemed to meet the need. Galambos was interested in the movement of the earthworm, not in its learning, but his work led me to explore the possibility of a Bechterevidian apparatus for the worm, the most recent version of which is shown in Figure 5. The animal lies in a narrow, covered trough, in the floor of which two electrodes are set, two loops of linen thread having been sewn previously into the dorsal musculature—one anteriorly and one posteriorly. A thread runs from the posterior loop to a fixed post, and another thread runs from the anterior loop to a spring which is attached to a second post. The spring is weak enough to permit withdrawal in response to shock, but strong enough to encourage return to the basal position when shock terminates. A third thread runs from the anterior loop to a kymograph on which response is recorded, as shown in the tracing. In this situation, brief shock elicits a response that looks very much like the flexion which shock to the limb elicits in the dog, and a neutral stimulus paired with shock may elicit a weak copy of the same response. Usually, however, the CR is a much more subtle affair which, though clearly visible to the naked eye, may fail entirely to activate the recording pen. After a good deal of time and effort devoted to the development of this apparatus, I have decided reluctantly that it should be abandoned in favor of an electronic technique which promises greater sensitivity.

Having emphasized the difficulties and disappointments which seem to be inherent in this exploratory work with relatively unfamiliar animals, I must note that it affords a rather special kind of enjoyment—a kind of enjoyment that I began to taste as an undergraduate in Schneirla's laboratory, but that I found only rarely in my subsequent work, which was more in accord with the spirit of the times. For many years, the field of animal learning has been dominated by a controversial, deductive spirit. Most of us have acted as though we knew all about the learning process, and as though the only purpose of our experiments was to

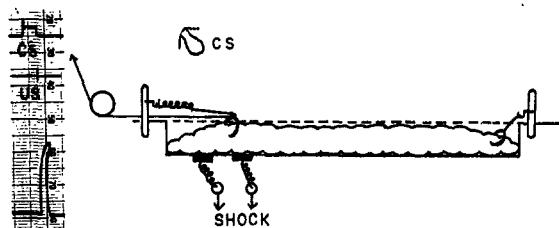


FIG. 5. Apparatus for the study of learning in the earthworm.

demonstrate the validity of our convictions. The controversy was fun, of course, and to some extent even productive—we managed certainly to accumulate a considerable amount of data on learning in the rat—but in my recent dealings with fish, and fly, and crab, and worm, I have come again to a kind of research that is at once more satisfying and more productive. Its function is inquiry, not proof. When I ask about the effects of partial reinforcement on resistance to extinction in the fish, or about the course of habit reversal in the crab, I have not the slightest notion what the answer will be; I can only wait eagerly for the outcome of my experiments. I am not suggesting, of course, that the same attitude cannot be taken in work with the rat. I am suggesting only that it comes more readily in work with the primitive animals, which are so far removed phylogenetically from those which have been taken as models by the various parties to the controversy—the lower mammals, on the one hand, and, on the other, man. Broadening the phylogenetic base of our work will facilitate the broadening of our outlook, and perhaps one day we shall be able to approach even the higher forms in the same spirit of discovery.

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